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IMAGE AND AFFECTION IN BEHAVIOR[1]

I

IN the thesis which I recently advanced[2] I had scant time to discuss two topics, which may seem to many to be stumbling-blocks in the way of a free passage from structuralism to *behaviorism.*

The first of these, and by all odds the more serious of the obstacles, is the "centrally aroused sensation" or "image." If thought goes on in terms of centrally aroused sensations, as is maintained by the majority of both structural and functional psychologists, we should have to admit that there is a serious limitation on the side of method in behaviorism. Imagery from Galton on has been the inner stronghold of a psychology based on introspection. All of the outer defenses might be given over to the enemy, but the cause could never be wholly lost as long as the pass (introspection) to this stronghold (image) could be maintained.

So well guarded is the image that it would seem almost foolhardy for us to make an attack upon it. If I did not perceive certain signs of weakening on the part of the garrison, I think I should agree with Professor Cattell that I am becoming too radical, and that I should better admit the claims of imagery and try to work out a scheme for behaviorism which will embrace the image. Suppose we consider this aspect of the question first: Does the inclusion of the image weaken the claims of the behaviorist? I am ready to admit that it does. Take a case like that ordinarily urged. Some one suggests in words that I borrow one thousand dollars and go abroad for a year. I think over the situation—the present condition of my research problems, my debts, whether I can leave my family, etc. I am in a brown study for days, trying to make up my mind. Now

[1] An address given before the Psychological Seminary of Columbia University, April 3, 1913.

[2] "Psychology as the Behaviorist Views It," *Psychological Review,* March, 1913.

the train of thoughts going on in my mind, according to the upholders of the image, has no adequate behavior counterpart while it is in transit. The behaviorist, observing me, might note that my appetite had departed, that I was smoking and drinking more than usual, and that I was distrait. Finally, experimental tests might show that my ability to make fine coordination had been seriously interfered with, that my dynamometric threshold was lowered, and so *ad infinitum*. The introspectionists would say that all of these tests failed to give anything like a complete record of my "mental content" or of the "totality of conscious processes." Indeed, they would urge that such tests have only an analogical reference. Only direct observation of the mental states themselves by the method of introspection will ever tell whether I am grieving over past sins or whether I am really trying to reach a decision about going abroad! If we grant this, and such an impulse is very strong, the behaviorist must content himself with this reflection: "I care not what goes on in his so-called mind; the important thing is that, given the stimulation (in this case a series of spoken words) it must produce response, or else modify responses which have been already initiated. This is the all-important thing and I will be content with it." In other words, he contents himself with observing the initial object (stimulation) and the end object (the reaction). Possibly the old saying "a half loaf is better than no bread at all" expresses the attitude the behaviorist ought to take; and yet I for one dislike to admit anything which may be construed as an admission of even partial defeat.

Feeling so, I prefer to attack rather than to remain upon the defensive. I spoke above of certain signs of disaffection and mutiny among the ranks of the faithful. These signs manifest themselves in three different ways: (1) The attempt on the part of Woodworth, Thorndike, and others to question the dogma of the image and to show that thought processes may go on independently of imagery—or, indeed, as I understand it, even independently of peripherally initiated processes. To this last contention I do not accede, as I shall undertake later to show. It is needless for me to discuss this phase of the problem at any length before this laboratory. (2) The failure on the part of the most earnest upholders of the doctrine of the centrally aroused sensation to obtain any objective experimental evidence of the presence of different image-types. I refer here to the researches of Angell and of Fernald. I think this admission paves the way for the complete dismissal of the image from psychology. Furthermore, I believe that most psychologists are willing to admit that introspection furnishes no guide for the determination of one's own image-type. In this field, above all others, introspec-

tion, if it is a legitimate method at all, ought to yield its best results. It is just here that it has failed, except in the case of a few fortunate men who seem to have become adept in the use of it. We who are less happy in its use must forever do without this wonderful Aladdin's lamp which, upon demand, illumines the dark places of the human mind. (3) The attempts even of the structuralists to reduce the so-called higher thought processes to groups of obscure organic processes. I have in mind the recent work on recognition, abstraction, etc.

All of these tendencies, initiated by the psychologists themselves, lead directly over to my principal contention, viz., that there are no centrally initiated processes.[3]

The environment in the widest sense forces the formation of habits. These are exhibited first in the organs which are most mobile: the arms, hands, fingers, legs, etc. By this I do not mean to imply that there is any fixed order in their formation. After such general bodily habits are well under way, speech habits begin. All of the recent work shows that these reach enormous complexity in a comparatively short time. Furthermore, as language habits become more and more complex there arise associations (neural) between words and acts. Behavior then takes on refinement: short cuts are formed, and finally words come to be, on occasion, substituted for acts. That is, a stimulus which, in early stages, would produce an act (and which will always do so under appropriate conditions) now produces merely a spoken word or a mere movement of the larynx (or of some other expressive organ).

When the stimulus produces either an *immediate overt response* (as, for example, when I tell John to go to the sideboard and get an apple, taking it for granted that he goes), or a *delayed overt response* (as, for example, when I ask an engineer to think out and make an apparatus for the conversion of salt water into sweet, which may consume years before overt action begins), we have examples of what one may call *explicit behavior*. In contrast to behavior of this type, which involves the larger musculature in a way plainly apparent to direct observation, we have behavior involving only the speech mechanisms (or the larger musculature in a minimal way; for example, bodily attitudes or sets). This form of behavior, for lack of a better name, I will call *implicit behavior*.[4] Where explicit behavior is delayed (*i. e.*, where deliberation ensues), the intervening

[3] I may have to grant a few sporadic cases of imagery to him who will not be otherwise convinced, but I insist that the images of such a one are sporadic, and as unnecessary to his well-being and *well-thinking* as a few hairs more or less on his head.

[4] It may be said in passing that the explicit and implicit forms of behavior referred to throughout the paper are acquired and not congenital.

time between stimulus and response is given over to implicit behavior (to "thought processes").

Now it is this type of implicit behavior that the introspectionist claims as his own and denies to us because its neural seat is cortical and because it goes on without adequate bodily portrayal. Why in psychology the stage for the neural drama was ever transferred from periphery to cortex must remain somewhat of a mystery. The old idea of strict localization of brain function is in part responsible. I feel, however, that religious convictions are even more largely responsible for it. I do not mean that the men originally responsible for the transfer were aware of this religious tendency at all. When the psychologist threw away the soul he compromised with his conscience by setting up a "mind" which was to remain always hidden and difficult of access.[5] The transfer from periphery to cortex has been the incentive for driving psychology into vain and fruitless searches of the unknown and unknowable. I am quite sure that if the idea of the image had never taken such firm hold upon us we would never have originated the notion that we are seeking to explain consciousness. We would have been content to study the very tangible phenomena of the growth and control of explicit and implicit habits.

It is implied in my words that there exists or ought to exist a method of observing implicit behavior. There is none at present. The larynx, I believe, is the seat of most of the phenomena. If its movements could be adequately portrayed we should obtain a record similar in character to that of the phonogram.[6] Certainly nothing so definite as this could be obtained, but we should get a record, at least, which would largely reveal the subject's word-habits, which, if I am not mistaken, make up the bulk of the implicit forms of behavior.

Now it is admitted by all of us that words spoken or faintly articulated belong really in the realm of behavior as much as do movements of the arms and legs. If implicit behavior can be shown to consist of nothing but word movements (or expressive movements of the word-type) the behavior of the human being as a whole is as open to objective observation and control as is the behavior of the lowest organism.[7]

[5] The tendency to make the brain itself something more than a mechanism for coordinating incoming and outgoing impulses has been very strong among psychologists, and even among psychologically inclined neurologists.

[6] I have been trying to find out whether any of the spoken phonographic records can be read by experts in that work. I have not been able to ascertain this information, but I am sure there is nothing inherently difficult about the problem. Records of laryngeal movements could likewise be read directly.

[7] It is implied here and elsewhere in my position that there is no scientific

II

Affection is the other stumbling-block in the way of our main thesis. It is needless for us to enter into a lengthy discussion of the various views about affection. It is sufficient to call attention to the generally accepted position that affection is a mental process distinct from cognition. Both Angell and Titchener in this country admit the independence of the two. In Germany, likewise, with the exception of the followers of Stumpf, the independence is admitted. Indeed, as is well known, Wundt and his pupils are attempting to introduce into affection the same wealth of detail they have already succeeded in bringing into cognition. I refer to the addition to the elemental processes of pleasure-pain, those of strain-relaxation, excitement-calm.

In maintaining his position as to the independence of the two processes, Titchener states that affection and sensation are closely similar in the following respects. Both possess certain common attributes, viz., *quality, intensity, duration.* Sensation possesses the additional attribute of *clearness,* which affection lacks. "The lack of the attribute of clearness is sufficient in itself to differentiate affection from sensation; a process that can not be made the object of attention is radically different, and must play a radically different part in consciousness, from a process which is held and enhanced by attention." Furthermore, the lack of *clearness* distinguishes affection from organic sensation—the cognitive processes with which it is most closely allied. On the whole, while sensation and affection are closely allied, "the difference is so great that we have no choice but to rank affection in human psychology as a second type of mental element, distinct from sensation."

Adherents of the view that affection is merely an attribute of sensation have not been lacking. Külpe has been given credit for demolishing this assumption.

There remains the view in contrast to the one first outlined, advanced principally by Stump and accepted and amended by Helen Thompson Woolley, viz., that affection is really organic sensation. The theory as advanced by Stumpf is all but unintelligible in view of the fact that the simplification that he obtains by his reduction is more than offset by the complexity he introduces when he states

value (or at most only temporary and provisional value) in self-observation. I sincerely believe that psychology would make far more rapid progress in the next twenty-five years than in all of its previous history, if it would conduct its experiments upon the assumption that the (normal) subject can hear but can not innervate the speech musculature beyond the point of making *silent whispers.* The same result might be obtained by working upon the assumption that the experimenter is deaf.

that the emotions, in addition to the complex of sensations, contain a mysterious "kernel." Mrs. Woolley, while rejecting the "kernel" hypothesis, insists that affection can be identified with sensation. She gives no clear reason for the solidarity and distinctness of the two groups, nor for the rather constant presence of the one or the other of these two groups. Stumpf no more than she meets these two points. My own view—which I advance as a theory, not as something introspectively ascertained or introspectively verifiable—may be stated somewhat as follows. I agree with Stumpf and with Woolley in holding that affection is an organic sensory response. Through lack of evidence, I reject the view that there are special pleasure and displeasure nerves. I admit, from the work of von Frey and that of Rivers and Head, that there are special cutaneous (proprioceptive) nerves which mediate pain. The first question which concerns us is how happens it that organic processes have become integrated into two such well-marked, solid groups known as pleasantness and unpleasantness? As they now stand they are really perceptions (objects) which at times may be examined as other objects, such as hunger, thirst, etc. At times they are as clear, and can be attended to in the same way, as the objects which arouse the exteroceptors. I have no sympathy with Titchener's view that these processes are never clear. It is a plain assumption, and a very weak one, arrived at largely in the interest of obtaining a structural differentiation between sensation and affection. At times these processes occur in conjunction with those from the eye and the ear, and since in certain situations the latter have very great stimulating value, the organic feature is extremely hard to observe. Under these conditions they are to some extent "obscure," as are all other organic processes, such as breathing, activity of the glands, circulation, etc. It is here, possibly, that Titchener gets his evidence for the view that they can not be attended to.

Secondly we are concerned with the question why the affective processes seem to be such constant accompaniments of other processes.

What I shall have to say in answer to these questions will not be surprising to any one who has followed the recent Freudian movement. I may preface my own remarks by saying that I do not follow this movement into all of its extravagances. I nevertheless feel that they have made good their main point concerning the sex references of all behavior. Since my first study of the movement I have been rather surprised that no one has connected pleasantness with the activity of the receptors stimulated by tumescence and unpleasantness with those stimulated by a shrinkage of the sex

organs.[8] To those who have inherent objections to admitting that the esthetic, artistic, and religious sides of life are at bottom sexual, this view will not sound convincing. I shall not attempt to develop the point further at the present time. I find in the hypothesis, however, sufficient reasons for taking the theoretical views (1) that "affection" is mediated by enteroceptors, as is hunger, thirst, etc.; (2) that there should be two well-marked groups of such sense processes which possess the solidarity, distinctness, and unity claimed for them by those who hold that affections are elementary; (3) that one or the other should usually be present—actually serving as "personal evaluators of experience"; (4) that their observation should be easy at times and difficult at others.

This view makes them open, as are all other forms of behavior, to objective investigation. You will tell me that expressive methods have already failed to show any constant physiological processes occurring in conjunction with the examination of "pleasant" and "unpleasant" objects. I have worked for years upon the expressive methods and no one will admit their failure in the past more readily than I. My present feeling is that we have taken our plethysmograms from the wrong organs. Whether there are too many technical difficulties in the way of the objective registration of the many delicate changes in the sex organs (circulation, secretion, etc.), remains for the future to decide.

Having thus summarily dismissed the image and the affective elements, I crave permission to restate the essential contention of the behaviorist. It is this: the world of the physicist, the biologist, and the psychologist is the same, a world consisting of objects—their interests center around different objects, to be sure, but the method of observation of these objects is not essentially different in the three branches of science. Given increased accuracy and scope

[8] The whole area involved in sex functions embraces a much wider zone than that of the sex organs proper. The erogenous areas are in infancy widely distributed throughout the body surfaces. Only gradually does the sex organ come to be looked upon as the focus of sex experience. Even in the case of most adults certain of these primitive zones remain functional, as, for example, the nipples, etc. The receptors lying in such areas are stimulated by the reflex motor processes initiated by the primary stimulus (*i. e.*, the object under observation).

[There are two things which possibly ought to be said in connection with this view. In the first place it is not essential to my contention that the above vague suggestion should be true. *It is essential to our position to have affection reducible to sense processes.* It is even more probable that the mechanism is glandular; that very slight increase in the secretion products gives us the one group; checking, or decreasing the secretion, probably the other. Finally it may be said that such a view is entirely independent of the ultimate fate of the Freudian movement.]

of technique, and the behaviorist will be able to give a complete account of a subject's behavior both as regards *immediate response* to stimulation, which is effected through the larger muscles; *delayed response,* which is effected through the same muscles (so-called action after deliberation)—these two forms comprising what I have called *explicit behavior;* and the more elusive types, such as the movements of the larynx, which go on in cases where action upon stimulation is delayed (so-called thought processes). This latter form of behavior, which manifests itself chiefly in movements of the larynx, but which may go on in (to the eye) imperceptible form, in the fingers, hands, and body as a whole, I should call *implicit* behavior. For years to come, possibly always, we shall have to content ourselves with experimental observation and control of explicit behavior. I have a very decided conviction, though, that not many years will pass before implicit behavior will likewise yield to experimental treatment.

Possibly the most immediate result of the acceptance of the behaviorist's view will be the elimination of self-observation and of the introspective reports resulting from such a method.

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A NEW ALGEBRA OF IMPLICATIONS AND SOME CONSEQUENCES[1]

THE development of the algebra of logic has done more than emphasize the close relation of logic and mathematics. It has helped to show the possibility of an ideal development of pure mathematics in general, free—or nearly free—from tacit assumptions, parsimonious in its postulates, and absolutely rigorous in its methods of proof. In this ideal development, the algebra of implications, or "calculus of propositions," appears as the organon of proof in general, and hence as the necessary first step. The work of Russell and Whitehead and others has called attention to this method of procedure.[2] It is the logical outcome of the denial that mathematics must appeal to "construction" or any other empirical datum, once its postulates are laid down.

From this point of view, the drawing of conclusions is not a process in which premises retire into somebody's reasoning faculty and emerge in the form of the result; nor is the conclusion obtained

[1] Read in brief before the American Mathematical Society, San Francisco Section, October 26, 1912.

[2] "Principia Mathematica," Whitehead and Russell, intends to exhibit just this development of at least the fundamental branches of mathematics.

through any subtle appeal to the perceptual character of space or of collections of marbles or arrays of fingers. Proof takes place through the collusion of two factors; first, postulates or propositions of the particular mathematical system in hand; secondly, postulates or theorems which state implication relations between premises of that logical or mathematical type and the desired conclusion. A mathematical operation is ideally no more than this: the substitution of the variables or functions of variables of the particular system—say, of cardinal number—for the logical variables in some proposition about implications. This proposition is more than a rule for inference; when the substitution is made, it *states* the implication. The result is the statement of what the variables or functions of the cardinal number system imply—a proposition in cardinal arithmetic. This result is not strictly the theorem to be proved, but only the statement that certain expressions or relations of variables imply certain others. Thus pure mathematics does not seek to prove theorems, but only, in the last analysis, that certain postulates imply certain theorems. And the proposition which states the particular implication relation—in more general form, because its variables have a wider range of meaning—is itself a mathematical proposition, in the algebra of logic.

Pure mathematics is not concerned with the truth either of postulates or of theorems: so much is an old story. But just here a curious reservation seems necessary. Modern geometry—Euclidean or non-Euclidean—is not concerned with the truth either of postulates or of theorems, but it *is* concerned with the fact that the postulates *truly imply* the theorems. It would seem, then, that pure mathematics must concern itself with the *truth* of the *propositions in logic* which state, in general form, the implications in question. Since the logic, also, does not prove its theorems, but only proves that its postulates imply them—that they are true if the postulates are true—this concern is, finally, with the truth of postulates in the logic.

It may be objected that the logic, like any pure mathematics, is concerned only with "mathematical consistency." If so, it must be borne in mind that the algebra of implications ceases, in a sense, to be pure and becomes applied when its propositions are used in proving anything. Indeed, the attempt to separate formal consistency and material truth is, in the case of the logic, peculiarly difficult. For while other branches find their organon of proof in the logic, this discipline supplies its own. Hence, if this system is formally consistent, but contains a primitive proposition which is materially false, we shall have *false proofs* as well as materially false statements of implications, within the logic itself. Also a materially

false theorem—either truly or falsely implied by the postulates—may, when used as a premise, lead to other false propositions; and when used as a *rule of inference,* to further false *proofs.* Thus a single materially false assumption in the logic might produce a numerous progeny early in life; and a "mathematically consistent" algebra of implications might still contain the basis for false proofs within itself, as well as in other branches.

Whoever has any interest in the rigorous development of pure mathematics may offer as warm a welcome to non-Euclidean geometry as to Euclidean, but can only view with alarm any algebra of logic whose postulates do not accord with the nature of valid inference.

Up to date, various sets of postulates for the algebra of logic have been offered, but all of the noteworthy ones produce mathematically equivalent systems, or systems which accord with the others so far as they go.[3] All of these which are complete contain theorems which have no application to our ordinary processes of reasoning and seem absurd to common sense.[4] The implication which figures in all such systems is what Mr. Russell has called "material implication," the difference of which from valid inference is undeniable.

The algebra to be proposed in this paper is applicable, in every proposition, to ordinary "sound reasoning," and it excludes all those

[3] Exception should be made of the system of Mr. H. MacColl, set forth in "Symbolic Logic and its Applications" (see also various articles in *Mind*).

Mrs. Ladd-Franklin's algebra makes inconsistency or exclusion the fundamental relation instead of implication. Nevertheless, any postulate or theorem of that system can be translated, *e. g.,* into a proposition of "Principia Mahtematica," and *vice versa,* provided the variables in both cases represent propositions. Mrs. Ladd-Franklin urges advantages of her algebra over those based on implication (see "Implication and Existence in Logic," *Philosophical Review,* Vol. XXI., pages 641–665), but these advantages are psychological and methodological rather than logical.

The system to be proposed in this paper has some points in common with the "formal implication" introduced by Mr. Russell in "Principles of Mathematics" and now included in "Principia Mathematica" (Vol. 1, page 132 and following), but it has also many points of difference. The most notable of these are: (1) Formal implication is based upon "material implication," and is immensely more complicated than the system to be proposed. (2) In formal implication, the negation of $(x) : \phi x \cdot \supset \cdot \psi x$ is equivalent to $(Ex) : \sim (\phi x \cdot \supset \cdot \psi x)$. In the proposed system $(x) : \phi x \cdot \supset \cdot \psi x$ is only a particular case of $p \supset q$ and its negative is simply $\sim (p \supset q)$. (3) As a consequence of (2) the proposed system does not treat, *e. g.,* "All men are mortals" as a case of implication, $(x) : \phi x \cdot \supset \cdot \psi x$, but as a relation of class inclusion.

[4] For a partial list of such theorems see "Interesting Theorems in Symbolic Logic," this JOURNAL, Vol. X., page 239. In that paper the number of such theorems is referred to as "apparently infinite." That it is actually infinite can be proved.

theorems of material implication which are not so applicable. It has, consonantly, a different meaning of "implies," and proceeds from somewhat different assumptions. For the sake of brevity, this proposed system will be referred to as "strict implication."

In order to make clear the different significance of strict implication from any system of material implication, it is necessary to introduce here some fairly tedious considerations. Material implication was preceded by, and historically grew out of, the "calculus of classes." But the algebra of classes contains an ambiguity which, comparatively harmless in itself, becomes serious when carried over into the algebra of implications.

In the algebra of classes, zero—in the usual notation—denotes the empty class or class with no members—the vertebrate invertebrates or the class of sea-serpents. But there are two kinds of such zero classes. Vertebrate invertebrates are impossible or *necessarily* non-existent: our fauna being what it is, the class of sea-serpents has no members. The class of Mondays that are Tuesdays is necessarily a zero class: the class of fair Mondays is a zero class in any month when it happens to rain every Monday.

Two further facts about the zero class are here worthy of attention. If $ac' = 0$ (if there are no a's which are not-c's), then all a's are c's. If ac' and $a'c$ are both zero classes, then a and c are equivalent or identical. They have the same extension, include the same members. If, now, all the Mondays are rainy and all the rainy days are Mondays, at some time, the class Mondays is identical with the class rainy days. But this relation is not a "necessary" one, since neither of the inclusions in this identity is a "necessary" inclusion. But consider a different identity. Let a be the class Mondays and c the days following Sundays. Then $ac' = 0$ and $a'c = 0$, $(ac' + a'c = 0)$, of necessity. All Mondays are days following Sundays and all days following Sundays are Mondays. We have here a necessary or definitive identity, an equivalence of *meaning* between "Monday" and "day following Sunday." The former identity is not of this type. If all Mondays are rainy days and all rainy days are Mondays, still that is no definition either of Monday or of rainy weather.

The defining relation (or pair of relations) is a relation of intension or meaning. To specify it requires the determination of two classes that are *necessarily* empty. This kind of class might be represented by equating it with a new symbol, -1. Such definitive or intensional equivalences are *not* specified when only extensional identity—the pair of $= 0$ relations—has been established.

The other point of importance is that the zero class is contained in every class. Since no Mondays are Tuesdays, the Mondays which

are Tuesdays are also Fridays. If a is Mondays, b, Tuesdays, and c, Fridays, $(ab)c'=0$. It is also the case here that $ab=-1$. When it rains every Monday, all fair Mondays fall on the eighteenth. If a is Mondays, b, fair days, and c, the eighteenth, then $(ab)c'=0$, since $ab=0$. The questions here involved as to the legitimacy of inferring $(ab)c'=0$ from $ab=-1$, or from $ab=0$, we must not here pause to discuss. The important point is to note what the inclusion of the zero class in every class means when we pass to the algebra of implications.

The transition may be made as follows: The ordinary hypothetical proposition, "If A is B, C is D," may be taken to state either an inclusion of classes or an implication. If a represent the class of cases in which A is B, and c, the cases in which C is D, then the proposition states that class a is included in class c, or $ac'=0$. This same equation may also represent the fact that "A is B" implies "C is D." At once we have a new interpretation of the algebra in which it becomes the usual algebra of implications—material implication. The further details of this transition are familiar.

Let us now suppose that "A is B" is a false proposition. Then the class of cases in which A is B is (within the "universe of discourse") an empty class. $a=0$. If $a=0$, $ac'=0$, whatever proposition c may be. If a is false, a implies any proposition, c. If we suppose "C is D," c, to be true, its negative, c', is false. If $c'=0$, $ac'=0$, whatever proposition a may be. If c is true, any proposition, a, implies c. "Napoleon was an ancorite" implies "fresh vegetables are poison." The class of cases in which Napoleon was an ancorite is a zero class and is thus contained in the class of cases in which fresh vegetables are poison, or in any other class.

This is certainly not an implication relation of a sort which ought to figure in mathematical proof. Euclid's parallel postulate or Lobachevski's postulate about coplanars is—one or other of them—false. Nevertheless, he errs who would take either postulate to imply anything and everything. Logical consequences follow regardless of truth or falsity of premises.

Suppose we consider the meaning, for the algebra of implications, of $ac'=-1$. The cases of a which are not cases of c are *necessarily* an empty class; the case in which A is B, but C is not D is logically impossible. This means, if a, then necessarily c. The truth of "A is B" in conjunction with the falsity of "C is D" is impossible. This is exactly the situation when "C is D" can validly be inferred from "A is B." We may now add that if $a=0$ ("A is B" is false), then $ac'=0$: there is no case in which "A is B" is true, but "C is D" is false, since there is no case in which "A is B" is true. But $ac'=-1$ does not follow. If a (A is B) *were* true,

ac' might *not* be an empty class: the situation in which "A is B" would be true, but "C is D" false, may *not* be an *impossible* situation. "C is D" can not be inferred from "A is B" merely because "A is B" is false.

We have here, in $ac' = -1$, a new—or, rather, old and usual—meaning of "implies," and the basis of a new algebra of implications. The detailed criticism of material implication and its reconstruction as strict implication may be omitted in favor of results.

In order to avoid such expressions as $ac = 0$ and $ac = -1$, it will prove convenient to give them equivalents. $ac = 0$ means "It is false that a and c are both true"; hence also, "At least one of the propositions, a and c, is, in fact, false." This last expression is usually symbolized as $a' + c' = 1$, or simply $a' + c'$. $ac' = -1$ means "It is impossible that a and c be true together"; hence also, "At least one of the propositions, a and c, is, of necessity, false. This relation may be symbolized as $a' \vee c'$. The distinction between $a' + c'$ and $a' \vee c'$, or between $a + c$ and $a \vee c$, is this: $a \vee c$ represents a dilemma whose horns are a and c; $a + c$ has a wider meaning and covers such expressions as "Either Napoleon was not an anchorite or fresh vegetables are poison." This last is no "necessary" relation. It breaks down before the test of contrary to fact supposition.[5]

We may now exhibit the primitive ideas and postulates for the proposed system of Strict Implication.

Primitive Ideas

1. *Propositions or propositional functions,* symbolized by p, q, r, etc.

A propositional function is an expression, containing a variable or variables, which becomes a proposition when a value of the variable or variables is assigned. It is one of the advantages of Strict Implication that propositions and propositional functions obey the same laws. We reason in the same "modes" about "x is a man" and "Socrates is a man."

2. *Negation.* $\sim p$ symbolizes the negation of p or "p is false."

3. *Implication.* $p \supset q$ symbolizes "q can be inferred from p" or "p (strictly) implies q."

4. *Product.* pq symbolizes the logical product of p and q, or the proposition which asserts both p and q; "p is true and q is true."

5. *Logical Constants.* Negation is a logical constant, and any relation which obtains between propositions or propositional func-

[5] For a discussion of the ambiguity of "either—or" propositions in relation to the algebra, see "Implication and the Algebra of Logic," C. I. Lewis, *Mind,* N. S., No. 84.

tions as such is a logical constant. Implication and product are such; other relations will be defined.

Postulates

P. 1. $(p=q) \supset [(p \supset q)(q \supset p)]$.
P. 2. $[(p \supset q)(q \supset p)] \supset (p=q)$.
P. 3. $(p \vee q) = (\sim p \supset q)$. Definition of the dilemmatic disjunction, $p \vee q, -pq' = -1$.
P. 4. $(p+q) = \sim (\sim p \sim q)$. Definition of the non-dilemmatic disjunction, $p+q, -pq' = 0$.
P. 5. $(p+p) \supset p$.
P. 6. $(p \vee q) \supset (q \vee p)$.
P. 7. $qp \supset pq$.
P. 8. $pq \supset p$.
P. 9. $p \supset \sim (\sim p)$.
P. 10. $(p \vee q) \supset (p+q)$. The dilemmatic disjunction of two propositions implies their non-dilemmatic disjunction as well. This implication is not reversible.
P. 11. $[p \vee (q \vee r)] \supset [q \vee (p \vee r)]$.
P. 12. $(q \supset r) \supset [(p \supset q) \supset (p \supset r)]$.

(Adjoined remarks in the above are, of course, no part of the postulates.)

P. 13. If p is asserted and $p \supset q$ is asserted, q may be asserted.
P. 14. If p and q are separately asserted, pq may be asserted.
P. 15. Any expression which contains only propositions, or propositional functions, and logical constants is itself a proposition, or propositional function.
P. 16. Any proposition or propositional function may be substituted for p or q or r, etc., in any postulate or theorem.

The last four postulates are the principles of operation of the system. The first two practically restate what is already contained in the primitive ideas of implication and product. The last two determine the range of the variables p, q, r, etc.

Optional postulates which might be added to our list are:

P. 17. $[p(qr)] \supset [q(pr)]$.
P. 18. $(q \supset r) \supset [(p+q) \supset (p+r)]$.
P. 19. $[p \vee (q+r)] \supset [q \vee (p+r)]$.

Of these, the first two have only infrequent and unimportant consequences. P. 19 has highly important consequences. The truth of every one of these turns upon the question as to what would be true in case logical principles were contradicted by facts. The truth of the postulate is doubtful and its significance chiefly epistemological.

It may be remarked in passing that P. 19 and its consequences well exemplify the fact that a formal postulate may sometimes serve to conceal rather than to express an important principle.

The entire system of strict implication could be developed from a somewhat smaller number of postulates, by a different choice of primitive ideas. But the set given above has compensating advantages.

Among the immediate consequences of these postulates, the following are selected as those which offer some difficulty of deduction but are important for further work. The order in which they are set down indicates, in general, the order of their simplest proof.

P. 20. $(p=q) \supset (p \supset q)$.
P. 21. $(p=q) \supset (q \supset p)$.
P. 22. $(\sim p \supset q) \supset (\sim q \supset p)$.
P. 23. $p \supset (p+q)$.
P. 24. $p \supset p$.
P. 25. $\sim (\sim p) \supset p$.
P. 26. $(p \supset q) \supset (\sim p \vee q)$.
P. 27. $(p+q) \supset (q+p)$.
P. 28. $(p \vee p) \supset p$.
P. 29. $(\sim p \supset p) \supset p$.
P. 30. $(p \supset q) \supset \sim (p \sim q)$.

P. 28, P. 29, and P. 30 state implications which are *not*, in strict implication, reversible; in any system of material implication they would be reversible. This difference is of considerable importance. The general character of further consequences of these postulates is indicated by the fact that strict implication includes all theorems of material implication which are consonant with the meaning of "implies" which we have chosen, and many other significant theorems which do not appear in material implication. Strict implication excludes all theorems of material implication which are false for ordinary inference. Besides excluding such doubtful theorems, strict implication makes clear the meaning of many others which seem doubtful because of the ambiguity of $p+q$, in systems of material implication, if read "either p or q." To take only two examples: (1) $\sim (p+q) \supset (\sim p+q)$;[6] (2) $\sim (p+q) \supset (p+\sim q)$:[7] both these theorems become clear when we discover that the disjunctions involved will not be dilemmas,—that the theorems do not hold in the form (1) $\sim (p \vee q) \supset (\sim p \vee q)$ and (2) $\sim (p \vee q) \supset (p \vee \sim q)$.

Strict implication has, also, positive results which are equally important. These may be brought out by considering P. 1 and P. 2. Objection might be taken to these postulates on the ground that they

[6] "Principia Mathematica," *2.47.
[7] *Ibid.*, * 2.48.

contain an undefined expression $(p=q)$ which is not mentioned among the primitive ideas. P. 3 and P. 4 are expressions of this type, and all such might be referred to as "definitions." The curious fact is that it is not essential, in strict implication, that definitions should have any meaning; it is requisite only that we should be able to get their implications. And these may be had, for any definition, by applying P. 1. If we substitute, in P. 1 $(p \vee q)$, for p and $(\sim p \supset q)$ for q, we shall have,

$$[(p \vee q) = (\sim p \supset q)] \supset \{[(p \vee q) \supset (\sim p \supset q)][(\sim p \supset q) \supset (p \vee q)]\}.$$

The first part of this expression is, now, the "definition," P. 3. The second part, which P. 3 is here asserted to imply, is a pair of implications. By using P. 7 and P. 8 upon P. 1, it could be shown how we may get either of these implications separately (see P. 20 and P. 21). That is, the "definition," P. 3, merely as an undefined expression containing functions of p and q, implies these two theorems, (1) $(p \vee q) \supset (\sim p \supset q)$, (2) $(\sim p \supset q) \supset (p \vee q)$. The only use to which any "definition" $(p=q)$ may be put, is to get from p to q or from q to p. This is properly done by using one of the implications $(p \supset q)$ and $(q \supset p)$. Thus "definitions" could be dispensed with altogether, and replaced by the pairs of implications which we get from them by P. 1. If this procedure were adopted, P. 1 and P. 2 would be superfluous.

The use made of definitions in any system of material implication, and in mathematics generally, is to substitute the defining expression for the defined, or *vice versa*, at any point in the "work." This procedure has been defended on the ground that the two expressions represent the same mathematical entity. But it is extremely doubtful if the only reason for any definition is that we may have various symbolic representations of the same thing. Definitions would then seem a useless violation of the principle of parsimony. Especially this is true where the defining expression is not symbolically simpler than the defined. In strict implication, and in any mathematical system which used it as an organon of proof, such substitution of expressions equivalent by definition is wholly unnecessary. Instead, one of the pairs of implications involved is made use of, and is treated exactly like any other implication. This brings one step nearer the ideal of mathematical work as consisting only in the substitution for variables of values of variables, or of other variables which may be regarded as values.

An alternative treatment of definitions would be to add to the number of our primitive ideas, the relation "is logically equivalent to" or "means the same as," symbolized by $=$. This procedure

avoids various possible objections to "expressions without meaning" and yet retains the advantage that substitution of equivalent expressions is unnecessary. Adopting this method, we can then make a somewhat spectacular deduction from P. 1 and P. 2. If we substitute in P. 2, the product qr, for q, we have (theorem 1):

$$[(p \supset qr)(qr \supset p)] \supset (p = qr).$$

Substituting, in this theorem, $(p = q)$ for p, $(p \supset q)$ for q, and $(q \supset p)$ for r, we have:

$$[\{(p = q) \supset [(p \supset q)(q \supset p)]\}\{[(p \supset q)(q \supset p)] \supset (p = q)\}] \supset \{(p = q) = [(p \supset q)(q \supset p)]\}.$$

The expression within the first pair of braces is now P. 1; within the second pair, P. 2. Hence this complicated expression may be read: Theorem 1 states that the product, (P. 1)(P. 2), implies as a theorem:

$$(p = q) = [(p \supset q)(q \supset p)].$$

This theorem may be read: "p means the same as q" means the same as "p implies q and q implies p." (The use of postulates P. 13–P. 16 in this proof is tacit.)

It would be unsafe to call this theorem a definition of the defining relation. Let us be satisfied to say that we have deduced an expression which makes clearer the meaning of that logical equivalence which was taken as a primitive indefinable.

This theorem is an interesting puzzle, but is unnecessary to strict implication, since the only use which would be made of it would be to derive its implications, which are P. 1 and P. 2. Other equivalences can, in strict implication, be deduced. For example, if we substitute q for p and p for q in P. 6, we have $(q \vee p) \supset (p \vee q)$. Without substitution, P. 6 reads $(p \vee q) \supset q \vee p)$. These two together imply, by P. 2, $(p \vee q) = (q \vee p)$. A dilemma means the same thing when the order of its members is reversed. This theorem is definitive of the dilemma just so far as the expressions, $(p \vee q)$ and $(q \vee p)$, are different. The theorem $(p \supset q) = (\sim p \vee q)$ can also be deduced. If we can not derive definitions, at least we derive equivalences of meaning, of which definitions are cases. Is it not possible that the usual treatment of definitions involves some confusion between mathematical and psychological requirements?

If what is set forth in the above paragraphs be correct, it throws considerable light on the nature of definitions in mathematics. Any definition is an equivalence of meaning between two expressions,—a reciprocal implication when "implies" has the meaning which figures in valid inference. "$p = q$ Def." means $(pq' = -1)$ and

$(p'q = -1)$. Definitions are not assumptions fundamentally different in kind from other postulates. They need not stand outside the mathematical system as sanction for substitutions, but should appear *in* the system and be used exactly as other postulates. Such equivalences can frequently be deduced, though the psychological requirement, that expressions must be clear in meaning when first introduced, limits the usefulness of such deductions. Definitions could be dispensed with, without materially altering the system, if proper implications were assumed.

It should be remarked, however, that strict implication does not stand or fall with this treatment of definition which it makes possible. The more usual method might have been followed in developing the system.

In leaving the subject of equivalences, it is well to note the difference between $(p = q)$ in strict implication and the "equivalence" ("Principia Mathematica," $p \equiv q$) which figures in material implication. This last is not an equivalence of meaning, but only of "truth value." $(p \equiv q)$ means only, "p and q are either both true or both false." This relation holds for $(p = q)$, but the defining relation can not be got out of it. For this reason, "equal by definition" is, in material implication, a primitive idea which needs to be distinguished from equivalence in general. The fact that equivalence of *truth value* is reciprocal *material* implication, while equivalence of *meaning* is reciprocal *strict* implication, throws more light upon the meaning of "implies" in the different systems. Which of these accords with the nature of inference, is a question of some importance for pure mathematics, if any ideal development from "logical constants" is to be attempted.

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DISCUSSION

"EVERYBODY'S WORLD" AND THE WILL TO BELIEVE

I AM not sure that these few lines, elicited by Mr. Adams's courteous paper,[1] should come under the head of "discussion." They are not intended as a refutation of what he has said, but, rather, as an explanation of one or two points on which I do not seem to have made my meaning clear.

But, first, I should confess that I regret having headed a chapter in my recent book with the words: "The World of the New Realism,"

[1] This JOURNAL, Vol. X., page 186.

as though there were but one. I have just been reviewing the literature of the last decade on realism, and have been so much impressed with the very significant divergences of opinion among realists keen enough to have an independent opinion, that I deplore any form of expression which might create the delusive impression that a "school" of new realists has the right to existence, and to recognition as a "school." There appears to be some danger that the expression "the new realism" may degenerate into the rallying-cry of a party, and may be accepted by those who disapprove of the present current of philosophical thought in England and America as a definitely fixed philosophy which may without more ado be made the object of attack of all and several. There are at least a score of realists writing at the present day, of whom, I think, no one has a better right to be called "new" than any other one, though some undoubtedly bring forward more novelties than do some others. How far individual opinions held by one or more of these writers are directly traceable to their realism is a fair question. Certainly no one of them should be made responsible for the opinions of all the others, and it is doubtful whether any one of them can justly be made responsible for all the opinions of any one of the others. I, for my part, do not wish to be made responsible for any debts save those contracted by myself.

Now, in calling the world phenomenon, I have not meant to say anything derogatory to the world, to its reality, to its order, to its system. To me it is a world of minds, as well as a world of material things. Of material things we have, I believe, what may justly be called a direct knowledge. Of minds other than our own, I believe our knowledge to be inferential. In saying, in the words quoted by Mr. Adams, that "we are relieved of the burden of a hopeless search for a reality wholly different in nature from the homely realities with which we are brought face to face every day," I had in mind those who distinguish sharply between appearance and reality and refuse to look for reality in what I call appearances, *i. e.*, in direct experience. I had the same thought in mind in saying "we are at the very heart of things, or as much so as it is conceivable that we should be," and supposed the context would make my meaning clear. I had no intention of denying that our knowledge, both of the material world and of minds, may be indefinitely increased; nor did I mean to assert that the hopes of those who look to find in the world evidences of a cosmic mind are to be regarded as illusive. The reasons which have been advanced for cherishing such hopes have been many and diverse. Some of them date back to a time long preceding the dispute between the realist and the idealist in the modern sense of those terms. They have not been regarded as incompatible with the existence of a material world, independent of perception, and not

properly to be called mental. Those who urge them, in our day, at least, usually feel that they are traveling beyond the confines of exact and indubitable evidence, nor do they expect to be able to convince those of an opposite mind as even the reluctant may be convinced within the confines of the more limited field of exact science.

I have said in my book that "Everybody's World" is imperfectly illuminated, but, nevertheless, maintained that certain features of it appear unmistakable. That the realist should adjust himself "to what is definitely known of reality" seems to me a reasonable demand; that he should adjust himself to nothing else appears at least disputable. I can see no reason why realism, as such, should bind his hands in this respect. The one city of refuge whose gates are closed to him is that built by the idealist—the stronghold of the idealist in the strict metaphysical sense of the word, and in no other sense. And if the realist's indulging a larger hope lays him open to the charge of transfiguring "the system of things given in our common experience," I suppose he must plead guilty, only begging that his offense be not confounded with that of certain others who would transfigure the system of things in quite other senses. He still holds to a physical world independent of perception, in a rather definite sense of those words, and to minds that know that world. And in permitting himself to transcend the limits of indubitable evidence in embracing a view of the system of things which takes account of the conservation of "human interests and values," he may differ more or less widely either with other realists or with idealists, and he may differ with them either in principle or in detail.

Thus, he is not at one with the realist to whom realism implies what has rather unhappily been termed naturalism. On the other hand, he is not at one with those who would transmute physical things into ideas, into mind-stuff, or into will. He is not at one with the philosopher who would make of minds and of things unreal appearances of a colorless Absolute which is neither mental nor physical. He distrusts the reasonings of the thinker whose dialectic would develop a universe by expanding the "internal meaning" of a song. If he is of a cautious turn of mind, he will accept with much hesitation enthusiastic statements touching his own power to create the truth and reality of the universe. Such transmutations of "Everybody's World" he can not accept.

Nor can he find that in all the philosophies which transmute the world, as is just above indicated, the conservation of "human interests and values" is really taken into account at all. Of provision for such in the philosophy of Berkeley there can be no question. It does not, however, appear to enter into the reckoning of the particular philosophers whom I have adduced as maintaining that the physical

world is "mind-stuff" or is "will." It can manifestly have no place in a doctrine in which human beings as well as human interests and values are infected with self-contradictions and can only contribute to reality in so far as they "cease." Is there any such conservation, in an unequivocal sense of the word, in the world attained by the expansion of the "internal meaning" of the song? I can find none whatever, though I am aware that others think they do. The cheerful pragmatic doctrine does make room for it—its protagonist was a realist—and in this respect it may be classed with the idealism of Berkeley, and contrasted with the other philosophies examined.

The moral of all which seems to me to be that, in making up our minds what sort of a world this or that writer believes our world to be, we must not ask merely whether he is a realist or an idealist. Idealists differ vastly among themselves. So do realists—even new realists. I ought to have made this clear in my book, if I have not done so. And if any idealist does in his philosophy provide unequivocally for the conservation of human interests and values, I am inclined to think that he is moved to do it by much the same considerations that influence the realist who makes the same provision, however different the paths which they seem to follow. In so far they ought not to be out of sympathy with each other.

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REVIEWS AND ABSTRACTS OF LITERATURE

Das Problem der Vererbung "erworbener Eigenschaften." RICHARD SEMON. Leipzig: Wilhelm Engelmann. 1912. Pp. viii + 203.

It is about a generation since Weismann precipitated the famous controversy over the "inheritance of acquired characters." Prior to that time such a mode of inheritance was generally accepted without question; indeed, the distinction drawn by Weismann between "germinal" and "acquired" characters had not even occurred to most biologists. The problem was promptly recognized as one of far-reaching importance. Herbert Spencer rightly saw that the triumph of this new point of view meant the collapse of some of his most cherished theories. For some years the controversy was waged fiercely. But at that time little appeal could be made to experiment, though Brown-Séquard's guinea-pigs long did valiant service on the one side and Weismann's dock-tailed mice on the other.

Weismann's skill as a dialectician, together with the real scarcity of decisive evidence on the other side, led to a strong trend of opinion in his favor. This trend gained further support from the brilliant studies of the cell which held the attention of biologists during the years when the controversy on "acquired characters" was keenest. One discovery after

another seemed to speak for the immutability of the germinal substance and the permanent individuality of its constituent parts.

Then came the unearthing of Mendel's celebrated experiments, and the formulation of the "mutation theory" of De Vries. Experimental Mendelism attracted hosts of enthusiastic followers, who busied themselves with elaborating the Mendelian formula to the *n*th power. And an immensely important mass of facts they have undoubtedly brought to light. To this dominant school of investigators, any suggestion that the old question of "acquired characters" was still deserving of an experimental test seemed reactionary or worse. "Lamarckism" to many of this school of thought was beyond the pale of scientific discussion altogether, being hardly more respectable than the alleged phenomena exhibited at a spiritualistic séance.

But, fortunately, a small group of investigators have not been deterred by considerations of "respectability," and now, after some fifteen or twenty years of experimentation, they have amassed a body of evidence which can no longer be ignored or treated lightly. *Evidence for what?* Certainly for the "inheritance of acquired characters" in the sense in which that expression was employed at the commencement of the controversy. But to many the time has come for discarding that rather threadbare expression altogether, partly because it is, in its nature, ambiguous, partly because Weismann and his followers have progressively restricted its meaning so as to exclude each new fact which they have been compelled to accept.

A few years ago, Semon brought together in a comprehensive way the most important data supporting this mode of inheritance in a paper of some 80 pages.[1] Very recently, he has amplified this earlier work and extended its scope. As a mere summary of a certain important group of biological facts, this little book is invaluable. It is most complete in its record of those experiments which have had for their object a direct attack upon the main problem which we are here considering. But nature's own experiments have in some cases received full consideration. Evidence has been drawn, for example, from the thickening of the skin on the soles of the feet, a condition already evident in the fetus, and from the diurnal rhythm of leaf-closing in certain plants, which is independent of individual experience.

Evidence, we may ask again, of what? Semon himself formulates his problem as follows: "May we suppose that, under favorable circumstances, through reactions evoked in the parent body, the hereditary potencies of the germ-cells (genotypical constitution), and thereby the reaction-mode of the offspring, may become modified in the same direction as the modifications appearing in the parents?" (pp. 9, 10). This may sound like a clumsy and not very intelligible way of asking whether or not acquired characters are inherited. But real differences are expressed. It is not an inhertance of the "character" itself which is contended for, but an altered *disposition,* which leads the "character" to develop spontaneously

[1] "Der Stand der Frage nach der Vererbung erworbener Eigenschaften," *Fortschritte der naturwissenschaftlichen Forschung,* II. Band, 1910.

at a given time or place. The "character" is merely an outward mark or symbol, through which the response of the organism to some stimulus manifests itself. The absence of such an outward indication in the parent does not necessarily imply the lack of any sort of reaction on the part of its living tissues. On the other hand, the presence of such an outward manifestation, though indicating some sort of reaction to a stimulus, on the part of the organism affected, does not necessarily imply that this reaction occurred at a time when the germ-cells could be influenced. To state the matter somewhat paradoxically, we may have the inheritance of an "acquired character" which was not manifest in the parent at all; while, on the other hand, a manifest character may have been impressed upon the parent at a period when any transmission was out of question (pp. 7, 8). Here, and elsewhere throughout the book, Semon makes use of Tower's apparent demonstration of the existence of a particularly impressionable period in the life of the germ-cells. Influences which are brought to bear during this period may lead to perceptible results in the offspring, while influences acting at any other time will probably have no appreciable effect. This is one explanation which Semon offers for the admittedly large proportion of cases in which modifications of the parent body leave no trace in the offspring.

It is plain to any one who follows the present work that the problem dealt with is one of much greater complexity than had been supposed at the time when it was first formulated by Weismann. Indeed, it is well known that Weismann's own conception of the "continuity of the germplasm" soon lost all claim to simplicity, but annexed one subsidiary hypothesis after another, as fast as data were produced which were incompatible with the earlier versions. Weismann was forced to admit not only the reality of a general influence upon the germ-cells of conditions affecting the "soma" or body, but to recognize the possibility that the former might be influenced locally and partially, resulting in definitely restricted effects upon the offspring. Such effects would not necessarily resemble, however, the original effects upon the parent, and even if they should, *there would be no inheritance of acquired characters.* For such instances of the parallel modification of parent and offspring, through the influence of external conditions, Weismann made use of a hypothesis to which the name of "parallel induction" was later given by Detto. This is the simple assumption[2] that the germ-cells are directly reached by the same physical influences, *e. g.,* temperature or nutrition, which bring about the modification of the parent body. Plainly, in such an event, it would be improper to speak of *inheritance* at all. This hypothesis has been used very freely by Weismann and others, and notably by Tower, who has brought forward some seemingly strong evidence in its support.

Among the most convincing parts of Semon's discussion are those directed against this interpretation of the phenomena at hand. He rightly contends that in some of the best authenticated cases of the repetition of parental modifications in the offspring, the possibility of a simultaneous

[2] The simplicity is more apparent than real, as the present reviewer, among others, has pointed out. (See *American Naturalist,* February, 1911, page 96.)

influence upon the germ-cells is practically excluded. Consider, for example, the effects of optic stimuli upon a salamander, or of temperature stimuli upon a mammal, or again of habituation to contact with mankind on the part of a wild species.

And even in those cases where the same physical agent could conceivably influence both the soma and the germ-cells, there are serious physiological difficulties in the way of thus accounting for identical specific effects upon parent and offspring. Here, as elsewhere, it is likely that the germ-cells are affected, not immediately, but secondarily, through a reaction evoked in the parental organism.

Semon has devoted considerable space to answering some of the older arguments against the "inheritance of acquired characters" which were based upon negative evidence, *i. e.*, the failure to detect such an inheritance in what would seem to be crucial cases. As regards mutilation experiments, for example, his answers are various. Perhaps, he says, some of these experiments would have yielded positive results if performed during the impressionable period of the germ-cells. And, after all, we must remember that a mutilation is not a reaction to a stimulus on the part of the organism, but a mere passive effect. The typical response to an injury, when such occurs, is regeneration. And here he points to certain experiments of Kammerer's upon the ascidian *Ciona,* in which it was found that the enlargement of the siphons, after amputation, repeated itself in the succeeding generation (pp. 58, 59).

Two seemingly formidable objections, which have been raised of late to the inheritance of parental modifications, are derived from recent work in experimental breeding. The first of these is the observed fact that "pure recessives," in cases of Mendelian inheritance, appear to be as pure, when extracted after many generations of hybridization, as they are at the outset. Why have not the "dominant" soma characters of the parent affected the eggs or sperm cells? Semon's attempt to meet this difficulty is, it must be confessed, not entirely satisfying, since it rests upon an assumption which does not seem wholly warranted. At other points in his discussion, however, he contends that "morphogenic excitations (those conditioned by the mere presence of parts) are, as a rule, perhaps everywhere, too weak to bring about an induction of the germ-cells" (p. 171). The most effective modifying influences for the latter are *active reactions* of the parent body to this or that external stimulus. "Functional excitations" (*i. e.*, such as arise from the use and disuse of parts) stand, in Semon's opinion, intermediate between the other two classes.

The second of these difficulties is one which has been made much of by Johannsen, namely, that the artificial selection of ordinary quantitative variations, particularly within a "pure line" (strain of uniform heredity composition) appears to be entirely inefficacious. Since the differences which appear within a pure line are, it is assumed, due wholly to the effect of chance differences in the environment of the various individuals, this fact is naturally offered as an additional argument against the "inheritance of acquired characters." The reply is that long-con-

tinued breeding is perhaps what is necessary in order to produce any visible results in such cases, and furthermore, that the selection of "quantitative" or "fluctuating" variations has, in reality, shown itself to be efficacious in the hands of such able breeders as Cuénot and Castle. Finally, negative results of this sort have no conclusive weight. There are confessedly many other cases of parental modification which appear to be without any visible effect upon the offspring, however long repeated.

In a controversy like the present one, where there are several interwoven problems in dispute, and where much of the dispute relates to the definition of terms, it means little to assert that either one or the other side of the case has been "proved." We may, however, heartily concur in Semon's belief that henceforth "any theory of heredity or descent has to reckon with the empirically established factor of somatic inheritance" (p. 173), without, perhaps, wholly agreeing with him in his interpretation of this "somatic inheritance." Indeed, we may go further and add that the dogmatic and unqualified denial of the "inheritance of acquired characters," which is still frequently heard, implies not only an ignorance of many well-attested facts, but an unfamiliarity with the present status of the whole question discussed in the work before us.

It is hardly fair to close the review of even such an admirable book as this one of Semon's without pointing out what seem to be one or two weak points. To the reviewer it appears that the worth of the experimental evidence has not always been carefully weighed. Most of us will probably agree in our admiration for the amazing productivity and versatility of the Austrian zoologist Kammerer. Yet the very extent of the territory covered by this investigator within the space of comparatively few years would deter many of us from basing any weighty conclusions upon some of his unconfirmed experiments.

Again, the possibility does not seem to be fully considered by Semon that detailed correspondences between parent and offspring may not always imply the existence of a causal nexus as complicated as the phenomena which it links together. Pathology teaches us that the action of a single bacterial poison may produce quite an elaborate symptom-complex, requiring weeks for its full development. Why may not the transmission of a comparatively simple chemical substance from parent to offspring produce in the latter a "symptom-complex" which parallels in detail a similarly caused group of changes in the former?

Thus it might turn out that some of the known instances of "somatic inheritance" could be explained in relatively simple terms. Whether or not such an explanation is of general applicability may well be doubted, but it ought, at least, to be fully considered before phenomena of a less conceivable sort are invoked. In any case, however, it would seem a mere quibble to deny the use of the word "inheritance" to such a form of transmission.

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Introductory Philosophy; A Text-book for Colleges and High Schools. CHARLES A. DUBRAY. New York: Longmans, Green, and Company. 1912. Pp. xxi + 624.

This book is remarkable for the quantity of subject-matter it contains. Besides a General Introduction, it includes the following major parts: Empirical Psychology, Logic, Ethics, Epistemology, Cosmology, Rational Psychology, Theodicy, and Outlines of a History of Philosophy. Moreover, each of these subjects is presented in considerable detail.

Pedagogically this book is admirable for its clearness, conciseness, and definiteness. It contains information that can be learned and recited. But what a tremendous undertaking such a book is for both teacher and student! Would it not be far better for the majority of students to know less, to be given only the problems in closer or more apparent relation to human life and affairs, and to gain the enthusiasm for philosophy and psychology which comes from such association? Moreover, so much that is highly interesting either is spoiled by being condensed or has to be omitted from so concise a book. Of course, much can be said for a book that leaves the interesting matter to the teacher to relate; but those of us who were brought up on James's "Psychology" can not forget how interesting this subject can be made by a text-book alone. Yet in saying this I must add two qualifications: first, that I found the entire book interesting reading, and perhaps some beginners would, also; and secondly, that conciseness, clearness, and definiteness are pedagogically the major virtues in any text-book.

As to doctrine taught, it is difficult to say anything which is fair either to the book or to the reviewer, because of the multitude of problems presented and solved. In general, though the doctrine is true to scholasticism and to ecclesiastical tradition, it reveals not only a wide acquaintance with modern philosophy and psychology, but a most welcome spirit of fairness, gentleness, and open-mindedness even where it refutes or condemns. To me the least interesting, the least satisfactory, though hardly the least modern part, is the empirical psychology. Whereas the theory of knowledge seems excellent. Indeed I wish our Hegelian colleagues would read this division and take it thoroughly to heart. Though I have strong Aristotelian leanings, the "Cosmology" seems to me to conflict with fairly well established scientific conclusions. Likewise as to the "Theodicy," modern philosophy certainly has shown that the problems of God's existence, of his nature and of his relation to the world, are not as easy as scholasticism makes them to be, and that whoever really solves these problems must do so by other means than merely by the older metaphysical devices.

But, regarding the book as a whole, both as to its doctrine and its pedagogical traits, my feeling is not one of fault-finding with, but one of decided indebtedness to, the author.

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L'Ennui: étude psychologique. E. TARDIEU. Second edition. Paris: F. Alcan. 1913. Pp. ii + 284.

This book, now in its second edition, deals with a mental state which may be considered a border-line condition between the normal and the abnormal. Ennui may not be well defined, but is described as "the equivalent of repose" and "the condition for work." All occasions and times give rise to ennui. A business or occupation is a detestable servitude and is described as an "*enuui en permanence,*" but some professions, *e. g.,* journalism, politics, acting, art, literature, are less so than others, because in them there is constant change. The work is cleverly written, and throughout there are numerous wise or cynical or satirical (the interpretation probably depending upon the reader's mood) aphorisms, such as that regarding business quoted above and the following: "*s'ennuer à deux, tel est l'idéal conjugal,*" and "*l'ennui est le demon de la femme.*" Although a chapter is devoted to the consideration of remedies, it is apparent that few can ever follow out the suggested treatment and become normal. Although not strictly scientific, its cleverness makes it well worth reading, for it is as entertaining as many recent novels. If a book of this character mirrors the author's mental state, there is sufficient in it to warrant the belief that he is a combination of misanthrope, philosopher, and humorist.

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JOURNALS AND NEW BOOKS

REVUE PHILOSOPHIQUE. February, 1913. *Les défenses psychiques* (pp. 113–134): DR. I. IOTEYKO. – This, a first article, is a study of pain starting out from the esthophylactic principle established by Sergi. *Origine des images symboliques* (pp. 135–155): É. BRÉNIER. – From the author's point of view, the image is the fixed element and the concept, the variable element. In the conflict between beliefs and realities, "the symbolic image is nothing but the most economical solution possible of this conflict and the return to equilibrium." *La valeur du pragmatisme* (pp. 156–183): H. ROBET. – "The originality of pragmatism is in having . . . subordinated the true and the real to the useful, in having made of value and of the good the keystone of epistemology, and in having placed in morality the foundations of metaphysics itself. . . . But a radical pragmatism is only possible on condition that one accepts by blind instinct the supreme ends of human life," and this is its great weakness. *Analyses et comptes rendus.* De Roberty, *Les concepts de la raison et les lois de l'univers:* G. H. LUQUET. J. M. Baldwin, *Thought and Things, Vol. III.:* A. LALANDE. Dr. É. Lask, *Die Lehre vom Urteil:* G. H. LUQUET. Dr. M. Montessori, *Le case dei bambini:* L. DUGAS. H. Dussauze, *Les règles esthétiques et les lois du sentiment:* C. LALO. É. Landry, *La théorie du rhythme et le rhythme du français déclamé:* L. M. É. Vendéen, *Principes du beau:* A. JOUSSIN. C. Bellangé, *Spinoza et la philosophie moderne:* R. BERROD.

G. Fuller, *The problem of evil in Plotinus:* C. Huit. *Revue des périodiques.*

McComas, Henry C. The Psychology of Religious Sects. New York: Fleming H. Revell Company. 1912. Pp. 235.

Major, David R. The Elements of Psychology. Columbus, O.: R. G. Adams and Company. 1913. Pp. xiii + 411.

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NOTES AND NEWS

The *Revue de Metaphysique et de Morale* comments as follows on the death of George Rodier at the age of forty-nine years.

"The premature death of George Rodier is a loss which will be keenly felt by all French students of ancient philosophy, by all who knew him and could appreciate his upright, straightforward character and the extent of his devotion to his friends, and, finally, by all those who saw with what extraordinary courage, in view of the cloud which darkened his life and which he strove to forget in his work, he bore the anxiety and suffering of his last years. As a student, at the Faculté des Lettres at Bordeaux, of Messieurs Espinas and O. Hamelin, he received from the instruction of the latter a stimulus which determined the philosophical trend of his whole life. One of his theses presented for the doctorate at the Sorbonne in 1891 dealt with *La physique de Straton de Lampsaque,* while the second, *De vi propria syllogismi* was a most remarkable piece of work which he afterwards translated into French and published in *l'Année Philosophique* for 1908. His peculiar ability as an historian of Greek philosophy became strikingly apparent in 1900 when he published Aristotle's *Traité de l'ame,* with a translation and commentary. Profound erudition, clear and penetrating exegesis applied to the most difficult and obscure portions of the original text, prudence and caution in its criticism and interpretation, and an intellectual honesty throughout are the indisputable merits of this important work.

"After teaching at Rochefort and Tarbes, Rodier was given a professorship at the *Facultés des Lettres* of Toulouse and Bordeaux, and, finally, in 1894 he was appointed to the chair left vacant by Victor Brochard at the Sorbonne. His works and the reputation which his teaching at Bordeaux had gained for him determined his appointment by the Faculté, an appointment seconded by the often expressed wish of Brochard.

"Besides his aforementioned works, Rodier wrote many important articles for various magazines: *La composition de la physique d'Aristote; Les mathématiques et la dialectique dans le système de Platon; Sur une des origines de la philosophie de Leibniz; La cohérence de la morale stoïcienne; L'évolution de la dialectique de Platon; Remarques sur le Philèbe, trois articles de la Revue des Études anciennes; Une revue générale de philosophie ancienne.*"